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ABSTRACT

Past studies linking schooling and career attainment to sibling position (family size, birth order, and spacing) are vulnerable to suspicions about omitted variables. Since they were based on cross sections of individuals from different families, they may have attributed to sibling position an influence belonging to unobserved parental attributes. This study retests the link between sibling position and achievement using a cross section of intra-family sibling differences. The alleged link is confirmed. Further, its pattern is consistent with the view that sibling position matters because of its straightforward effects on family time and commodity inputs into children. In conclusion, it is indicated that reductions in fertility should raise the average level and reduce the dispersion in schooling. Family size and other dimensions of sibling position have a less significant impact on final occupation. It appears that the main reason for the effect of fertility reduction on schooling is simply that it gives each child more inputs from his family. The tendency of lower fertility to raise schooling needs to be noted in any projection of the demand for higher education. (Author/AM)

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SIBLING POSITION AND ACHIEVEMENT

Peter H. Lindert

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
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SIBLING POSITION AND ACHIEVEMENT

PETER H. LINDERT

ABSTRACT

Past studies linking schooling and career attainment to sibling position (family size, birth order, spacing) are vulnerable to suspicions about omitted variables: being based on cross-sections of individuals from different families, they may have attributed to sibling position an influence belonging to unobserved parental attributes. This study retests the link between sibling position and achievement, using a cross-section of *intrafamily* sibling differences. The alleged link is confirmed. Further, its pattern is very consistent with the view that sibling position matters because of its straightforward effects on family time and commodity inputs into children.

Many studies have concluded that children raised in larger families tend to have lower IQs, leave school earlier, and end up in lower-status, lower-paying jobs—other things equal.[1] This result is usually interpreted as showing that larger family size strains parents' ability to devote time, energy, and money to each child. The strain-on-family-resources interpretation seems to receive further support from studies of the consequences of birth order.[2] First-borns seem to have

The author is Associate Professor of Economics, University of Wisconsin-Madison.

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1 For references to the vast empirical literature tying family size to subsequent achievement, see my earlier discussion paper [9].

2 For references to the debate over the effects of birth order, again see [9]. Most of the literature focuses on the alleged advantage of first-borns over all later-born siblings. Only with respect to achievement, and not IQ, emotional stability, or other variables, has it been argued that the last-borns have an advantage over middle-borns for given family size.

higher IQs and achievements than middle-born or last-born children, for any given family size. The last-borns tend to stay in school longer than middle-borns, though they have no IQ advantage. These patterns suggest that the middle-born children are disadvantaged by being forced to compete with siblings for parental resources throughout their childhood, while last-borns receive generous parental inputs only when they are in their teens, after the other siblings have left the household. The birth-order results thus point to the same strains on parental resources emphasized by authors of studies of the effects of family size.

The family-size results clearly imply that reductions of fertility would raise average achievement levels. They further imply that any reduction in the dispersion of family size, a common accompaniment to reductions in average family size, will tend to equalize the educational and career attainments of children. If these implications are correct, then the "baby bust" that began in the 1960's should improve both the average achievement and the economic equality of the children born in this era of low fertility, even aside from the tendency of this restriction on future labor supply to cut down the percentage pay premiums enjoyed by those with extra schooling and skills.

I. THE PROBLEM OF OMITTED VARIABLES

For all the attention devoted to the relationship of achievement to sibling position, the entire literature remains vulnerable to a single line of attack: other important variables have not been held constant, and any correlation between these omitted variables and sibling position (number of siblings, birth order, and child spacing) can yield misleading estimates of the impact of the latter. The most serious omitted variables are unobserved parental characteristics related to child orientation or tastes and ability for developing achievement in individual children. The danger of bias in estimates of the effects of sibling position can be seen most easily by supposing that sets of parents with the same observable attributes (for example, occupation, education, race, region, religion) differ in their trade-offs between fertility and inputs per child. It could well be that parents with greater tastes or abilities for grooming achievers than other parents with the same observed attributes tend to have fewer children in order to pour more resources into each child. If so, then studies showing that greater family size (or middle-sib position, which is correlated with family size) depresses achievement may really be showing only the relevance of unobserved parental tastes and abilities for grooming achievers. This difficulty in interpreting the family-size and birth-order results would cause little harm if it were simply noted. Unfortunately, by not noting it, studies of the "consequences" of family size may overstate the positive effect on child achievement of such exogenous antinatal influences as improved access to birth control, improved job opportunities for women, and tougher tax policies toward dependents. We

need to know how to divide up the apparent effects of family size on child achievement (holding constant the observable parental attributes) between the influence of unobserved parental attributes and all other influences on family size.

Until the problem of omitted parental variables is attacked with better tests, there exists an opportunity for a revisionist attack on the past studies that saw a link between family size (or birth order) and achievement. Pronatalists could raise the issue of omitted variables, add any of several suspicions about the data used in past studies, and advance the unresolved suggestion that being from a large family or being a later-born is not bad at all. Some initial steps have already been taken in this revisionist direction [7, 13].

The issue need not be left in such an uncertain state. The problem of unobserved parental attributes can be minimized by tests linking differences in achievement among siblings raised together to sibling positions and other variables. In what follows I shall (1) retest the link between sibling position and educational and career attainments, using a cross-section of pairs of siblings; and (2) suggest and test a simple explanation of the observed patterns relating sibling position to achievement, an explanation focusing on family time inputs into child development.

It turns out that the link between sibling position and achievement is significant even when age and parents' attributes have been held constant. One reason for the importance of sibling position appears to be the fact that it affects the time inputs into the individual child more strongly than do the ages, status, and labor force participation of the parents. The family-inputs index which helps to underline these points emerges as a variable that some studies of achievement might profitably use as a more efficient proxy for family-structure effects than the number of siblings.

II. THE NEW JERSEY SIBLING SAMPLE

A convincing test of whether and why sibling position matters to achievement requires a sample with data on intrafamily achievement differentials. The data should also allow one to make adjustments for age to take account of the fact that different cohorts of growing children experienced different access to schooling. A sample that meets these requirements is the sample of 1,087 siblings collected in 1963, when a Cornell Medical School team interviewed 312 senior male employees of a New Jersey utility company in search of information about the incidence of heart disease.[3] The 312 interview respondents

3 For a more detailed description of the sample and for extensive analysis of the homogeneity of the siblings in the sample, see Hermalin [5]. The elimination of observations with incomplete information narrowed the sample from 1,087 siblings, including the 312 respondents, to 1,008 siblings, including 189 respondents. Larger samples may

were asked, among other things, for the age, sex, educational attainment, and most recent occupation of each of their siblings, living or dead. The respondents themselves ranged in age from 55 to 61, and the ages of their siblings ranged from 31 to 81. The sibling sample thus consists of persons old enough that their formal schooling had been completed. Furthermore, only a dozen siblings were under age 40 in 1963, so that the boom in incomes and educational opportunity after Pearl Harbor (for example, the GI bill) must have had no effect on the schooling or first jobs of almost all of the sample.[4]

The nature of the New Jersey sibling sample does complicate the present inquiry in one respect, however. The interview respondents were clustered into the higher educational and occupational classes, being all long-term employees of a company with a high average skill level. This high average status need not be a problem in itself, but two other facts make it a problem. First, the respondents had higher average schooling and career achievement than the average levels they reported for their own siblings. Second, the likelihood that any given sibling in the sample was himself the interview respondent is closely related to his sibling position, and in particular to his family size. In the absence of any other information about an individual in the sample, we know that the probability of his being one of the high-achievement respondent group is $1/N$, where N is the number of siblings in his family. Hence, all only children were interview respondents, as were half the siblings from two-child families, one-third of the siblings from three-child families, and so forth. Thus in this sample there is an unavoidable relationship between family size and respondent status, and another relationship between respondent status and predicted achievement.

The selection bias involved in the sample design can be handled in various ways. The range of options can be seen more clearly by referring to the causal relationships portrayed in Figure 1. The first option is the "reduced form" strategy of arguing that the probability that an individual is the interview respondent for his family is itself simply one channel through which the true importance of sibling position for achievement shows up in this particular sample. Following this strategy, one would simply ignore who the respondents were and run regressions explaining achievements in terms of the variables on the left in Part A of Figure 1 (family background, sibling position, and sex). This procedure, of course, is likely to overestimate the importance of sibling

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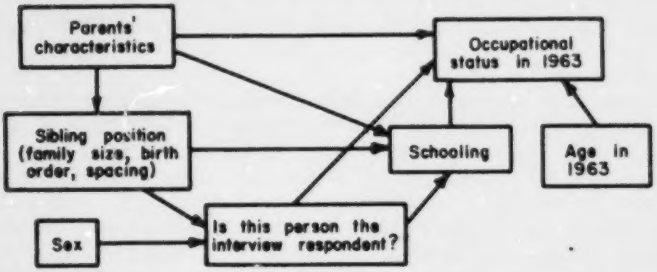
soon be available that meet the requirement of having data on the age, sex, education, and occupation of each adult sibling. One possibility, being explored by Hermalin, is that the original interview sheets of the 1965 Productive Americans survey will yield these data on all the grown children of several hundred elderly respondents. Another source of such data, apparently, will be the 1970 National Fertility Survey. These samples could be employed as a cross-check on the results reported here, but were not available at the time of writing.

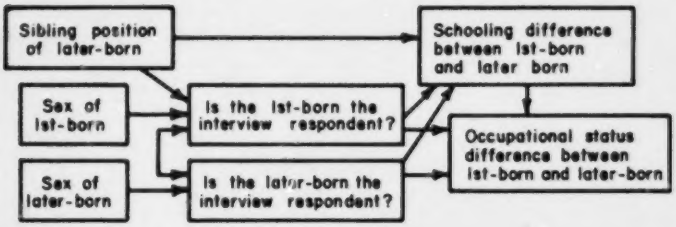
4 Unreported regressions showed that removing the 12 siblings under age 40 had no effect on intrafamily patterns of educational attainment.

FIGURE 1
INFLUENCES ON PREDICTED ACHIEVEMENT
IN THE NEW JERSEY SIBLING SAMPLE
A. Achievements by Individuals in Different Families



Note: The other possible arrows from left to right have been omitted because they failed to prove statistically significant.

B. Achievement Differentials Within Families


Sibling position of later-born
Schooling difference between 1st-born and later born
Sex of 1st-born
Is the 1st-born the interview respondent?
Occupational status difference between 1st-born and later-born
Sex of later-born
Is the later-born the interview respondent?

position and sex. The interview respondents were not a random sample of males, nor were the only-child respondents a random sample of only children, or the two-sibling respondents a random sample of persons from two-child families, and so forth. Such a strategy will be tried out below, but with the expectation that it overstates the importance of sibling position.

Two other testing options are likely to attribute less, and probably too little, importance to sibling position as a determinant of achievement. One is the device of adding a dummy variable for respondent status to the regressions explaining educational and career attainments. This is likely to understate both the unit impact and the statistical significance of sibling position. By mixing the respondents into the sample, this procedure is biased against sibling position by

implicitly denying that sibling position was an influence in the likelihood of an individual's being a respondent in this survey.

A better procedure is to run tests on the subsample of nonrespondents, thus keeping the nature of the respondents from biasing the estimates of the impact of sibling position. Still, a test on a sample of siblings who are not respondents is a stern one for the sibling-position hypotheses. It cuts down the representation of the small families over which family size was supposed to matter most. A test from a sample of nonrespondents is thus likely to underestimate the significance (t-statistics) of sibling position variables.

The best way of dealing with the special nature of the sample, however, is to run the kind of test that is also best for meeting the suspicions about omitted family variables raised by the past literature: examine *intrafamily* differentials in achievement. Part B of Figure 1 sketches a set of influences that could be tested by redefining all variables as differentials between the first-born and a later-born, to be employed over a sample consisting of all of the later-borns (non-first-borns) in the New Jersey sibling sample. Explaining intrafamily differences in achievement should give the most accurate indication of the importance of sibling position. The special nature of the group of interview respondents is less of a problem in an intrafamily test. Since family size is the same for each first-born as for each of his own siblings, a first-born and each later-born in the same family have the same likelihood of being the interview respondent working for the New Jersey utility (except to the extent that birth order truly affects achievement). Therefore regressions following the scheme of Part B in Figure 1 should reveal the role of sibling position with or without dummy variables identifying the interview respondents. These intrafamily tests, and the other testing options just mentioned, will be probed next.

III. ACHIEVEMENT PATTERNS: REGRESSION RESULTS

When all of the different ways of correcting for the special nature of the New Jersey sibling sample have been tried, it turns out that sibling position is at least as significant an influence on achievement as past literature implied, and for the simple reason that sharing family time and money with siblings is a drag on achievement. This conclusion emerges from results like those given in Tables 1 through 3. To survey these results, let us examine the three tables in order.

Whenever the schooling levels of individuals in different families are analyzed, as in Table 1, certain background variables prove unmistakably significant. Schooling levels are higher, the higher are the parents' schooling levels and occupational status. A broken-home background significantly reduces an individual's likely educational attainment. Sex and age, on the other hand, were not significant influences on schooling in this sample. Both sex and age looked significant only in those regressions where they were allowed to act as proxies for

TABLE 1

PATTERNS OF INDIVIDUAL EDUCATIONAL ATTAINMENT:
REGRESSION RESULTS FROM THE NEW JERSEY SIBLING SAMPLE
(Standard Errors in Parentheses)

Independent Variable	Regression Number (Dependent Variable Is *ED*)					
	(1) Coefficient	(2) Coefficient	(3) Coefficient	(4) Coefficient	(5) Coefficient	(6) Coefficient
Constant term	-1.815	1.198	1.233	-1.252	1.919	1.770
	(2.268)	(2.351)	(2.343)	(2.134)	(2.152)	(2.196)
MALE	.298**	.076	.078	.302**	.058	.063
	(.109)	(.116)	(.118)	(.108)	(.115)	(.117)
EDMA	.249**	.226**	.251**	.250**	.229**	.253**
	(.052)	(.060)	(.052)	(.052)	(.060)	(.052)
EDPA	.220**	.260**	.223**	.223**	.261**	.224**
	(.046)	(.053)	(.046)	(.046)	(.053)	(.045)
SEIPA	.018**	.018**	.018**	.018**	.018**	.018**
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
BROKEN	-.505**	-.582**	-.526**	-.482**	-.572**	-.505**
	(.172)	(.201)	(.170)	(.170)	(.199)	(.168)
AGE	.138	.035	.033	.152*	.039	.034
	(.078)	(.081)	(.081)	(.074)	(.075)	(.077)
AGESQ	-.0014*	-.0005	-.0005	-.0015*	-.0005	-.0004
	(.0007)	(.0007)	(.0007)	(.0007)	(.0006)	(.0007)
RESP	–	–	.636**	–	–	.680**
			(.140)			(.137)
SIBLOSS	–	–	–	-2.393**	-2.236**	-1.373*
				(.611)	(.968)	(.638)
ONLY	1.071**	–	.658*			
	(.294)		(.305)			
FIRST2	.742**	.718**	.512*			
	(.216)	(.316)	(.219)			

TABLE 1 *(Continued)*

Independent Variable	Regression Number (Dependent Variable Is *ED*)					
	(1) Coefficient	(2) Coefficient	(3) Coefficient	(4) Coefficient	(5) Coefficient	(6) Coefficient
FIRST3	.531*	.533	.410			
	(.238)	(.283)	(.237)			
FIRST45	.209	.248	.178			
	(.217)	(.232)	(.215)			
FIRST613	.638*	.625*	.589*			
	(.269)	(.287)	(.267)			
MID3	.613*	.644*	.551*			
	(.236)	(.263)	(.234)			
MID45CLO	.086	.007	.111			
	(.250)	(.264)	(.248)			
MID45WID	.250	.151	.194			
	(.176)	(.193)	(.175)			
LATTER	.454	.365	.369			
	(.215)	(.252)	(.214)			
LAST3	.436	.346	.329			
	(.238)	(.282)	(.237)			
LAST45	.480	.489	.387			
	(.221)	(.248)	(.220)			
LAST613	.455	.300	.315			
	(.276)	(.306)	(.275)			
$R^2_{adj.}$/SEE	.3074/1.5866	.3144/1.5334	.3210/1.5790	.3054/1.5889	.3128/1.5351	.3214/1.5705
Degrees of freedom	988	700	987	999	710	998
Sample	1008 persons	719 nonrespondents	1008 persons	1008 persons	719 nonrespondents	1008 persons

* Significant at the 5 percent level; ** significant at the 1 percent level.

Note: The coefficients on sibling-position binary variables show the impacts of these positions relative to middle children in families of six or more. Definitions of variables follow Table 2.

TABLE 2

PATTERNS OF INDIVIDUAL CAREER STATUS ATTAINMENT: REGRESSION RESULTS FROM THE NEW JERSEY SIBLING SAMPLE

Independent Variable	Regression Number (Dependent Variable is *SEI*)			
	(1)		(2)	
	Coefficient	Standard Error	Coefficient	Standard Error
Constant term	-90.16*	(40.26)	-81.72*	(38.57)
EDPA	1.91**	(0.71)	1.86**	(0.70)
EDMA	0.85	(0.81)	0.84	(0.81)
SEIPA	0.22**	(0.04)	0.22**	(0.04)
BROKEN	-7.40**	(2.77)	-7.54**	(2.73)
AGE	4.47**	(1.40)	4.25**	(1.34)
AGESQ	-0.04**	(0.01)	-0.04**	(0.01)
RESP	13.35**	(1.88)	13.59**	(1.84)
SIBLOSS	–	–	-14.18	(9.13)
ONLY	4.21	(4.08)		
FIRST2	5.38	(3.27)		
FIRST3	0.35	(3.81)		
FIRST45	3.65	(3.64)		
FIRST613	8.42	(4.47)		
MID3	4.91	(3.90)		
MID45CLO	-2.52	(4.05)		
MID45WID	-0.68	(2.80)		
LATTER	1.84	(3.29)		
LAST3	-0.87	(3.75)		
LAST45	-1.91	(3.56)		
LAST613	3.60	(4.89)		
$R^2_{adj.}$/SEE	.2688/20.20		.2708/20.17	
Degrees of freedom	639		650	
Sample	659 males		659 males	

* Significant at the 5 percent level; ** significant at the 1 percent level.

Definitions of Variables Used in Tables 1 and 2

Dependent Variables:

ED = a one-digit schooling code, ranging in values from 1, for less than an eighth grade education, to 8, for five or more years of college. (As explained in [9], an alternative scaling was also tried–one based on the logarithm of earnings predicted for levels of schooling–but it yielded results essentially identical to those reported for *ED*.)

SEI = a two-digit index of socioeconomic status, based on the individual's most recent occupation. The index number assignments were made by Hermalin.

Independent Variables:

MALE = 1 if the individual is a male, 0 if female.

EDMA = the mother's educational attainment = *ED* for the mother.

TABLE 2 (*Continued*)

EDPA = the father's educational attainment = *ED* for the father.
SEIPA = the socioeconomic status of the father's final occupation = *SEI* for the father.
BROKEN = dummy variable for a broken home during the individual's childhood = 1 if the parents were separated or divorced or if the interview respondent spent time in a foster home or orphanage.
AGE = the individual's age in 1963, in years.
AGESQ = *AGE* squared.
RESP = 1 if the individual is the interview respondent, 0 if it is one of his siblings.
SIBLOSS = index of reduction in time inputs received by the child because of his having siblings = 1 minus the index given for the appropriate sibling position in Table 4. The indices calculated for one-year spacing in Table 4 were used whenever average child spacing was less than 1.5 years; the two-year indices were used for spacing from 1.5 to 2.5; the three-year figures for spacing from 2.5 to 4.5; and the six-year figures for spacing over 4.5 years.
ONLY = 1 if the individual is an only child, 0 otherwise.
FIRST2, *FIRST3*, *FIRST45*, *FIRST613* = 1 when the individual is the first-born of two, three, four or five, and six through 13 children, respectively.
MID3, *MID45CLO*, *MID45WID* = 1 when the individual is an intermediate-born child of three children, or four or five children spaced more closely than two years apart, or of four or five children spaced two years or more apart, respectively.
LATTER, *LAST3*, *LAST45*, *LAST613* = 1 when the individual is the last-born of two, three, four or five, or six or more children, respectively.

interview respondent status, the respondents being all male and all over age 55.

Each of the specifications predicts that greater family size reduces the schooling expected for each sibling. The first three regressions also suggest that closer child spacing (for four- and five-child families) may be a disadvantage for educational attainment, but the spacing pattern was not statistically significant. As for birth order, the hypothesized disadvantage of being a middle child shows up among families with more than three children, but fails to pass significance tests.

All family background variables tend to explain schooling levels better than they explain occupational status late in life. The reason is that other events intervene between the completion of schooling and the peak of an individual's career. Accordingly, Table 2 shows that all background variables are less significant, and account for a smaller share of total variation, when it comes to explaining career status rather than schooling. Not surprisingly, age has much more to do with a person's occupational status than with his schooling. Respondent status is strongly significant as well, since the respondents were drawn from a fairly high-status occupational group. Sibling position, like the other background variables, is not statistically significant as a job-status determinant, though its coefficients are still generally of the "right" sign.

The best tests for resolving suspicions about omitted variables are the intrafamily regressions reported in Table 3. These regressions convert the models

TABLE 3

INTRAFAMILY DIFFERENCES IN EDUCATIONAL ATTAINMENT: REGRESSION RESULTS FROM THE NEW JERSEY SIBLING SAMPLE

Independent Variable	Regression Number (Dependent Variable Is *EDDIF*)					
	(1)		(2)		(3)	
	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Constant term	.440*	(.200)	-.198	(.152)	-.208	(.152)
*MALE*1	.253	(.146)	.275	(.245)	.528**	(.133)
MALEH	-.155	(.145)	-.144	(.145)	-.429**	(.134)
*RESP*1	.479*	(.191)	.521**	(.177)	–	–
RESPH	-.634**	(.180)	-.657**	(.178)	–	–
DIFINPUT	–	–	4.758**	(1.819)	4.983**	(1.813)
LATTER	-.112	(.261)				
*LAST*3	-.347	(.260)				
*LAST*45	-.719**	(.232)				
*LAST*613	-.428	(.280)				
*MID*3	-.355	(.259)				
*MID*45*CLO*	-.340	(.267)				
*MID*45*WID*	-.304	(.195)				
*SPACE*1*N*	-.065	(.043)				
$R^2_{adj.}$/SEE	.0785/1.7491		.0752/1.7522		.0396/1.7856	
Degrees of freedom	754		761		763	

* Significant at the 5 percent level; ** significant at the 1 percent level.

Sample: 767 later-born (not first-born) siblings from 269 families, each compared in achievement with the first-born from the same family.

Note: The sibling-position binary variables in Regression (1) yield coefficients comparing the impact of assuming each later-born position relative to being a middle child in a family with six or more children. Thus, for example, the coefficient on *LAST*613 indicates the extent to which the schooling advantage of first-borns over last-borns in these large families differs from (in this case, is less than) the advantage of first-borns over middle children in families of six or more children.

Definitions of Variables Used

Dependent Variable:

EDDIF = the first-born's educational advantage = the difference between the schooling level attained by the first-born sibling and that attained by one later-born sibling in the same family. The schooling level in this case is a one-digit code with values from 1, for less than an eighth grade education, to 8, for five or more years of college.

Independent Variables:

*MALE*1 = 1 if the first-born is male, 0 if female.

MALEH = 1 if the later-born to whom the observation applies is male, 0 if female.

*RESP*1 = 1 if the first-born is the (male) interview respondent working for the utility company in 1973, 0 otherwise. The function of this and the next variable is to quantify whatever distorting influences may have arisen from the facts that the interview

TABLE 3 (*Continued*)

respondents are a specially selected group of employees and that they may have systematically misreported their siblings' schooling.

RESPH = 1 if the later-born to whom the observation applies is the interview respondent, 0 otherwise.

DIFINPUT = the difference between the index of time inputs that the first-born child is predicted to have received and the same index for the later-born child, where the input indices are those given in Table 4. The figures calculated for one-year spacing were used whenever average child spacing was less than 1.5 years; the two-year figures were used for spacing from 1.5 to 2.5; the three-year figures for spacing from 2.5 to 4.5; and the six-year figure for spacing over 4.5 years.

LATTER = 1 if the later-born to which the observation applies is the second of two children.

LAST3 = 1 if the later-born is the last of three children, 0 otherwise.

LAST45 = 1 if the later-born is the last of four or the last of five children, 0 otherwise.

LAST613 = 1 if the later-born is the last of from six to 13 children (no families had more than 13 children), 0 otherwise.

MID3 = 1 if the later-born is the second of three children, 0 otherwise.

MID45CLO = 1 if the later-born is the middle (intermediate-born) child in a family of four or five children and the number of years in age to the next oldest sibling plus the number of years to the next youngest sibling is less than four, 0 otherwise.

MID45WID = 1 if the later-born is a middle child in a family of four or five children and the number of years separating the next oldest sibling and the next youngest sibling is greater than or equal to four, 0 otherwise.

SPACE1N = the average spacing between siblings in the same family = the years separating the oldest and the youngest in the family divided by (the number of siblings minus one).

of Table 1 into equivalent intrafamily differential form.[5]

One might expect that the intrafamily tests would be difficult ones for the sibling-position hypothesis to pass. When the dependent variable is a differential in the schooling of two siblings raised together, R^2 is going to be low. The dependent variable is a single digit, and some of the most powerful determinants of achievement across families—parents' education and occupational status, a broken-home history, and even completed family size—are of no help in accounting for intrafamily differentials. In view of the fact that family size was a more important determinant of schooling than were birth order and spacing, it would have been no surprise if Table 3 had reported only an insignificant influence of birth order on differences in schooling.

It is thus striking to find how much sibling position does seem to matter within families. Some predicted contrasts pass significance tests. First-borns end up with a significant edge in schooling over middle-borns of the same sex in families with six or more children (see first three terms in Regression (1) in

5 The analogy between the models of Table 1 and those of Table 3 would be closer if terms corresponding to *AGE* and *AGESQ* had been included in the intrafamily regressions of Table 3. Regressions with the appropriate age-difference variables were run, but were not reported in Table 3 since the age-difference variables were never significant.

Table 3, where the constant term represents effects relative to middle-borns among six or more siblings). Last-borns among four or five have an advantage over their eldest brothers and sisters that contrasts significantly with the disadvantage of middle-borns among six or more relative to their eldest siblings (*LAST*45 term in the same regression). Not significant, but of the "right" sign, are the advantages of the last-borns over middle-borns within large families of given size (*LAST*613 and *LAST*45 versus the *MID*45s). In other words, a tough intrafamily test of the importance of birth order tends to support the same arguments about the disadvantages of sibling crowding that underlie the belief in a disadvantage of large family size.

Thus far, however, we have been retesting only the hypothesis that sibling position (family size, birth order, and child spacing) matters to achievement. The underlying presumption that it matters because sibling crowding strains family resources has not been tested, since we lack a quantification of just how the strain on family resources is supposed to vary over family sizes, birth orders, and child spacing. We next develop a set of indices of family inputs received by children in different sibling positions. These indices will then be examined as influences on achievement, influences represented by the special variables *SIBLOSS* and *DIFINPUT* in Tables 1 through 3.

IV. THE LINK WITH FAMILY INPUT PATTERNS

There are many competing hypotheses about how sibling position affects achievement and other dimensions of child development. Some hypotheses feature differences in parental expectations by birth order. Others link family size with parental resort methods of behavior control that are external, autocratic, achievement-retarding, and labor-saving. Though the true origins of the influence of sibling position remain a legitimate subject for debate, most of the competing hypotheses agree on a *proximate* cause of whatever importance sibling position may seem to have. They share the belief that achievements (and some other personality traits) are influenced by the extent to which a child shares the time, emotional energy, and financial resources of adults with other children of similar age. It is generally believed, in other words, that a child receives from brothers and sisters less support for the development of career-relevant skills than he loses through their competition for the attention and support of adults.

Quantifying the above arguments about family inputs into a child requires using regression estimates from both time-use surveys and consumption surveys to calculate the expected effect of children in various sibling positions on family expenditures of time and money, given the attributes of the adults in the household. This procedure is complicated, time-consuming, and full of pitfalls, but reasonable estimates can be obtained. In what follows, I shall focus on the

patterns of apparent time inputs into children and shall not treat patterns of commodity inputs into children.

Time Inputs into Siblings

Using time-use surveys to infer how much time each child receives requires two steps. First, one must run regressions to determine how the total amounts of time spent by individual family members on child care vary with the numbers and ages of the children present. The second step is to rely on some key theoretical assumptions in order to convert the observed patterns of total child care into estimates of the amount of attention time received by each child per day or week or year. Theoretical assumptions are unavoidable in this second step because of two problems of interpretation. The first problem is that it is often hard to decide whether a given activity constitutes caring for or interacting with children. The other main problem is in allocating to an individual child a part of the total time reported as "child care."

An earlier discussion paper [9] has spelled out at length my choice of key assumptions for estimating the amount of time inputs received by each sibling per day for different days of the week and seasons of the year. These assumptions have been applied to regressions on total child-care time in a Cornell time-use survey of 1,296 Syracuse families in 1967–68.

Even without the aid of assumptions about the allocation of total child-care time among siblings, the regressions underline the importance of the ages as well as the numbers of children in determining parents' patterns of time use. The ages of the children dominate differences in child-care time, with infants contributing far more to daily or annual hours of child care than do children of school age. Extra numbers of children also raise the child-care burdens at a decreasing rate, as one would expect. These patterns hold both for the mother's time and for the total time contributed by all persons to caring for the children of one family. Attributes of the parents prove much less significant than the ages and numbers of children present as determinants of amounts of child-care time.[6]

If the number and ages of children are such dominant influences on the total amount of time devoted to child care, there is good reason to look more closely at estimates of how the amount of time going into an individual child is affected by the number and ages of his siblings. In addition to estimating these

6 The mother's age, her education, her work status, and the father's socioeconomic status all fail to exert statistically significant influences on the total amount of care time given to children of given number and ages. It appears that the only way in which more educated wives as a group manage to devote more time to each child is by having fewer children on the average. This finding contradicts the conclusion of other authors that more educated mothers spend more time with their children even for given ages and number of children [6, 8]. The appendix to [9] gives my reasons for concluding that their results are based on inappropropriately measured variables.

TABLE 4
PREDICTED CHILD-CARE TIME RECEIVED OVER 18 YEARS BY CHILDREN IN VARIOUS SIBLING POSITIONS, AS A FRACTION OF THAT PREDICTED FOR AN ONLY CHILD, BY YEARS OF SPACING BETWEEN ADJACENT CHILDREN
(Based on 1967–68 Data)

Child Spacing (Uniform)	Children Ever Born	Child's Birth Order				
		1st Born	2nd Born	3rd Born	4th Born	5th Born
	Only child	8227 hrs. (1.000)	–	–	–	–
1-year	2	6518 hrs. (.791)	6518 (.791)	–	–	–
	3	5528 (.672)	5393 (.656)	5528 (.672)	–	–
	4	5757 (.700)	5555 (.675)	5664 (.688)	5761 (.700)	–
2-year	2	6662 hrs. (.810)	6662 (.810)	–	–	–
	3	5690 (.692)	5435 (.661)	5698 (.693)	–	–
	4	5799 (.705)	5407 (.657)	5625 (.684)	5819 (.707)	–
3-year	2	6790 hrs. (.825)	6794 (.826)	–	–	–
	3	5853 (.711)	5468 (.665)	5867 (.713)	–	–
	4	5834 (.709)	5256 (.639)	5443 (.662)	6003 (.730)	–
6-year	2	7172 hrs. (.871)	7188 (.874)	–	–	–
	3	6338 (.770)	6049 (.735)	6604 (.803)	–	–
	4	6338 (.770)	5544 (.674)	5794 (.704)	6604 (.803)	–
	5	6338 (.770)	5544 (.674)	4959 (.603)	5794 (.704)	6604 (.803)

Source: [9], using regression results from the 1967–68 Syracuse time-use survey conducted by Kathryn E. Walker and others in the College of Human Ecology, Cornell University. I am indebted to Professor Walker for making the survey data available for the present study.
Notes: The figure in parentheses is the ratio of the estimated time received by this child to that received by an only child.

TABLE 4 (*Continued*)

Each estimate was derived by the following steps: (1) Regressions were run relating child-care time by all persons to the ages and numbers of children in the household and other variables. (2) For each child age-and-number variable, the coefficients from regressions for different parts of the year (nonsummer weekdays, summer weekdays, and weekends) were weighted by their shares of a year and summed to get an annual coefficient. (3) The net impact of each child on total child-care time in a given year was computed by adding and subtracting the coefficients just mentioned. For example, the net impact of a 5-year-old with a 1-year-old sibling would equal the coefficient for (2–5 year age group, two children in the home) plus that for (1-year-old, two children in the home) minus that for (1-year-old, the only one in the home). (4) These net impacts are then summed over the 18 years that each child spends in the household. (5) The 18-year net impact total is corrected by subtracting from that sibling's total impact on family child care the number of hours he or she spent taking care of younger siblings. This correction is necessary to keep the extra child care supplied by an older sibling from being counted as time spent on him by others. (6) The estimated net impact of a given child on total care of family members is a lower-bound estimate of the hours of care time from which that child received some benefit. The upper-bound estimate is the hours of care time he would receive as an only child. The next step is to calculate the mid-point between these two bounds. This "mid-range" estimate is what is presented in the table above. (To get the prior net-impact figure, double the mid-range estimate and subtract 8226 hours, the total care time for an only child.)

For families with five or more children, direct estimates could not be made because too few such families were represented in the sample. In order to include large families in the regressions using an index of time inputs, it was necessary to extrapolate values based on those for the largest families represented in this table. The rule for extrapolating was as follows: for all first-borns in larger families, use the index for the first-born in the largest family size represented here; for all last-borns, use the last born's index for the largest family here; similarly extrapolate for those of penultimate birth order; and for all others extrapolate the index for the second-born of the same average child spacing. The same rule could be used for the case of the six-year spacing, but it is unlikely that such spacing would occur in families with six or more children.

time inputs from various caretakers into each child each year, one must aggregate over caretakers and over the years of the child's life in a way that requires further assumptions. I have opted for the simplest aggregation procedure and have added up all of the hours received by a child from any person in any of the 18 years he is assumed to spend in his parents' household. The total number of hours for a child in each of several sibling positions are given in Table 4 and illustrated in Figure 2.

Several patterns emerge from Table 4's estimates of the time received by children in different sibling positions. In general, these patterns correspond to the simple formula mentioned at the start of this section: the presence of an extra sibling of similar or lower age reduces the amount of time (and attention) that a child receives. The appearance of a younger sibling lowers time inputs for a child in most sibling positions. It is interesting to note, though, that the estimates find this effect stronger for the arrival of the second and third child in the

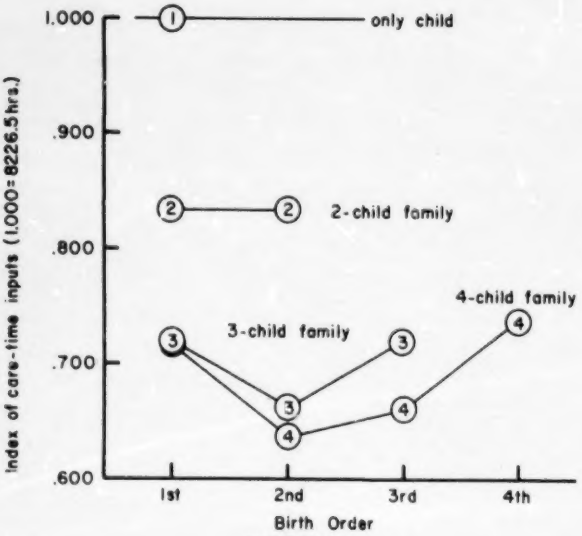


FIGURE 2
PREDICTED CHILD-CARE TIME RECEIVED OVER 18 YEARS BY CHILDREN IN VARIOUS SIBLING POSITIONS, AS A FRACTION OF THAT PREDICTED FOR AN ONLY CHILD: THE CASE OF THREE-YEAR CHILD SPACING

Source: Table 4

family than for the arrival of the fourth. It may be that a first-born or second-born suffers no further loss of attention from the appearance of the fourth and later siblings. Being the youngest of four or more siblings may similarly be no worse in this respect than being the latter of two or the last of three. Indeed, the estimates imply that the youngest of four receives more time inputs than the youngest of three, in part because the first-born is often old enough to help with the care of the fourth child. The table also asserts that middle-born children receive fewer hours of care than the eldest and youngest in all cases, primarily because they lack the opportunity to be the only child in the household at any time in their lives. The estimates also imply that wider child spacing provides more hours of attention for the eldest and the youngest, but has an ambiguous effect for the middle children. Finally, the estimates imply that a last-born receives at least as much care time as a first-born in families of the same size, by receiving enough more attention as a school-age child to make up for the

first-born's advantage in the preschool years before the arrival of siblings.

A slightly transformed version of the indices in Table 4 has been used in regressions in Tables 1 through 3 as a means of testing the hypothesis that sibling position affects achievement by affecting family inputs into the child. The slight transformation involved is the introduction of a new variable, *SIBLOSS*, which equals one minus the appropriate index shown in parentheses for each sibling position in Table 4. This transformation is intended to cover against uniform percentage errors in the deviation of the time inputs received by each non-only child from those received by an only child. *SIBLOSS*, in other words, is assumed to equal the fraction of input reduction from only-child norms times an unknown constant. As long as this assumption is correct, the value of the unknown constant is irrelevant to the ability of sibling position to help in explaining achievement differentials.

Family Time Inputs and Sibling Achievement

When indices of family time inputs are used as regressors in place of sibling-position variables, they prove to be a significant influence on schooling. This result is firmly established by tests on intrafamily schooling differences (*DIFINPUT* coefficients in Table 3) as well as by tests on individuals' schooling (*SIBLOSS* coefficients in Table 1).[7] The predicted-time-input index is also a

7 Adding an age differential (and the differential in the squares of ages) reduces the magnitude and significance of the *DIFINPUT* index. In a sample restricted to later-borns, the age difference between each sibling and the first-born is highly correlated with the *DIFINPUT* index, which predicts higher time inputs for a later-born child the farther his birth lags behind the first birth. Thus neither age nor the *DIFINPUT* index appeared significant in the unreported regressions that included both. This result is interpreted as showing that age differentials themselves do not belong in the model used for intrafamily testing. Since age proved insignificant in the comparison of individuals from different families, while *SIBLOSS* was significant (Table 1), it is viewed as an unimportant determinant of schooling differentials within families, and as a variable that just happens to be correlated with *DIFINPUT* in a sample of later-borns.

Other unreported regressions tested the hypothesis of Smelser and Stewart [11] that the schooling advantage of the first-born is greater in families with children of both sexes than in families where all were of the same sex. Regressions were run with the variable *ALLMALE* added to sets of variables like those in Table 3. The *ALLMALE* variable did have the negative coefficient that Smelser and Stewart would predict, suggesting that when boys are raised without sisters, the first-born has less advantage than when both sexes are represented. But the coefficient for *ALLMALE* was never statistically significant, leaving the issue unresolved. This interaction of sex with sibling position deserves further exploration, especially if the development of sex preselection technology leads to a shift toward first-born males followed by second-born females, as predicted by Westoff and Rindfuss [14]. If Smelser and Stewart are right, sex preselection could be a minor force tending to widen male-female differentials in occupational status through an effect of sibling position on schooling.

significant influence on career attainments if one views respondent status as determined in part by sibling position (unreported regressions), but not significant if respondent status is held constant (Regression (2) in Table 2). These results are all consistent with the hypothesis that sibling position affects achievement because it strains family inputs, even though one could still choose to view the outcome as a possible coincidence produced by strong correlation between the time-inputs index and some unspecified "true" influence on achievement.[8]

It should be noted, however, that the estimated relationship of schooling to sibling position fits the time-inputs predictions only broadly and not neatly. First-borns show an advantage over last-borns in cases where this was not predicted by the time-input indices. Whereas the indices predicted parity or a slight advantage of last-borns over first-borns, the regression results fit this prediction only for families of four or five children. For families of two, three, or six or more, the first-borns had a slight unpredicted advantage. This result cannot be explained by adding commodity input patterns on top of the time-input pattern since, as I have argued elsewhere, last-borns are predicted to receive a greater advantage in commodity inputs than in time inputs [9, pp. 18–20]. The better performance of first-borns than predicted is likely to have two explanations. First, each hour of time spent on a preschool child may have a greater impact on schooling and career than each hour at later ages, and first-borns, as noted, receive more hours than later-borns at preschool ages. Second, parents may simply be more relaxed about spending time and energy in developing children after the first.

The present results are consistent with another line of inquiry, despite what may seem like a contradiction. In their recent reexamination of "Birth Order and Intelligence Development," Zajonc and Markus [15] noted several relationships between sibling position and intelligence test scores. As with the present achievement patterns, larger family size meant lower scores, and middle-borns fell below first-borns for given family size. But unlike the achievement patterns, last-borns were found to have a much lower intelligence test score than earlier-borns for the same family size. Also unlike the achievement patterns, only children were not at any great advantage. Zajonc and Markus offered an index of home intellectual environment during childhood which was a transformation based on sibling position.

The patterns observed for intelligence are for the most part consistent with the view that sibling position is relevant to child development mainly through its

8 The present results are consistent with the quantification of the effect of family size on schooling by Beverly Duncan. Using regressions on the 1962 survey of Occupational Change in a Generation, she estimated that children in two-child families had on the average 0.6 years more final schooling than children in five-child families when age and six other variables were held constant [4, pp. 648-49]. This degree of impact of family size on schooling is also predicted by the present regressions containing terms for age or age differences, though larger effects of family size are predicted when age is not held constant (e.g., using *DIFINPUT* coefficients from Table 3).

effects on family time and commodity inputs into siblings, as long as one adds the plausible assumption that the inputs that shape intelligence are the inputs into the child's early, say preschool, development. Calculations like those summarized in Table 4 would show that the predicted pattern of time inputs by sibling position for the first six years of childhood match the patterns observed by Zajonc and Markus and others before them. Last-borns suffer in their early years, and only in their teens do they receive relatively generous attention and financial support. It seems reasonable to argue that they are indeed less intelligent (on tests) than earlier-borns, yet at the same time achieve more than middle-borns by receiving more encouragement to stay in school. The only aspect of the Zajonc-Markus patterns not predicted by the time-inputs index is the failure of the only child to score higher, though unobserved parental attributes may explain this.

V. CONCLUSIONS

There is a considerable body of evidence that reductions in fertility should raise the average level, and reduce the dispersion, in schooling. Family size and other dimensions of sibling position have a less significant impact on final occupation. It further appears that the main reason for the effect of fertility reduction on schooling is simply that it gives each child more inputs from his family. This family-input effect is reinforced by the tendency, which I have documented elsewhere, for reductions in aggregate fertility to ease the strain on public school inputs per child of school age.[9] The resulting higher level and lower dispersion of schooling should, in turn, tend to equalize incomes further by shifting the young-adult labor supply toward the higher-skill occupations, bidding down their pay advantage over the unskilled.

The tendency of lower fertility to raise schooling needs to be noted in any projections of the demand for higher education between now and the year 2000. It is now common knowledge that higher education is an industry strongly affected by movements in fertility. The college boom of the 1960s has already vanished, as was predicted by Allan Cartter [2] and others. Pessimism now rules the forecasts on demand for college, fed by the knowledge that the pronounced fertility decline will cut the size of the college-age cohort across the 1980s. This pessimism has been reinforced by a recent article by Stephen P. Dresch, which correctly notes that the economic returns from college will continue to be depressed by the glut of college-educated members of the baby-boom cohorts [3].

The results above suggest a partial offset to the tendency of demand for

9 The evidence linking the strain on public school expenditures per child to fertility is presented in [10, Section VI].

college to be depressed by shrinkage in the size of the age group, even when this demand is further lowered by a previous baby-boom glut of degree holders. This fertility reduction itself will raise the attendance rates for a cohort that has had better access to family and public-school inputs. Even in the face of possible declining rates of return on a college degree, parents will tend to follow through to some extent on their prior decision to cut fertility for the sake of assuring each child higher inputs and earning potential. A smaller-family cohort will tend to have a higher rate of college attendance, even if this tendency itself makes college less of an economic investment and more an act of consumption. This attendance effect needs to be considered, along with the more pessimistic arguments, in forecasting the demand for higher education.

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